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of Hansen. These, like Leverrier's tables of the inner planets, are now more than thirty years old. These tables have been compared with observations, and agree fairly well with those made during the century preceding their publication, but not with those made before or since that time. The theoretical value of the acceleration of the moon's longitude is $6''$; that found by Hansen from accounts of ancient total eclipses of the sun, $12''$. Newcomb, however, considers these accounts as unreliable, and, limiting himself to the Ptolemaic eclipses of the *Almagest* and the Arabian eclipses of the *Table Hakémité*, obtains the value $8''.3$, or, from the Arabian eclipses alone, $7''$,—a value but little greater than the theoretical value. Dr. Ginzell, from an extended examination of accounts of ancient and mediæval total eclipses of the sun, concludes that Hansen's value requires a change of only a little over $1''$. His solution, however, in reality depends upon the ancient eclipses alone. The only other theory of the moon comparable with Hansen's is that of Delaunay. This theory, however, is limited to a determination of the inequalities in the motion of the moon due to the action of the sun, on the hypothesis that the orbit of the earth is a pure ellipse, and differs from that of Hansen in that the inequalities determined are not expressed numerically, but only symbolically in terms of arbitrary constants.

While the co-efficients of the inequalities upon which Hansen's tables are based seem to be pretty well known, I am not aware that the tables themselves have been sufficiently checked, except by comparison with observations. Apparently the great desideratum now is a set of tables computed from Delaunay's theory in a completed form, or computed in some other way entirely independently of Hansen's. Until Hansen's tables are thus checked, it is questionable whether it can be safely said that the motion of the moon cannot be completely accounted for by the law of gravity.

The detection of the two satellites of Mars by Professor Hall may be considered the most interesting recent achievement in pure discovery. It was not till the discovery of these satellites that a means was offered for the accurate determination of the mass of that planet. No satellites of Venus and Mercury have as yet been detected, and the values at present assumed for the masses of those planets are very uncertain.

In 1788, just one hundred years ago, Laplace published his theory of Jupiter's satellites. This theory is still the basis of the tables now in use. Souillart's analytical theory of these satellites appeared in 1881. The numerical theory was completed only within the last year, and the tables therefrom remain still to be formed.

Bessel made a careful investigation of the orbit of Titan; but the general theory of the Saturnian system which he commenced, he did not live to finish. Our knowledge of the motions of Saturn's satellites, with the exception of Titan, was very meagre until the erection of the great equatorial at Washington. A difficulty in the determination of a correct theory of the motions of Saturn's satellites is the fact that there are a number of cases of approximate commensurability in the ratios of their mean motions. The most interesting case is that of Hyperion, whose mean motion is very nearly three-fourths that of Titan. In this case there is the additional difficulty that their distance from one another is only about one-seventh as great at conjunction as at opposition.

Our knowledge of the motions of the satellites of Uranus and Neptune depends almost entirely on the observations made at Washington. Quite accurate determinations of the masses of these two planets have been obtained. The large secular motion of the plane of Neptune's satellite, to which Marth has called attention, needs confirmation.

The number of the asteroids is so great that they have been the frequent subject of statistical investigation. The systematic grouping of the nodes and perihelia which exists was shown by Newcomb to be the effect of perturbation. Glauser finds that the grouping of the nodes on the ecliptic is a result of a nearly uniform distribution on the orbit of Jupiter. Professor Newton had previously found that the mean plane of the asteroid orbits lies nearer to the plane of Jupiter's orbit than to the orbit plane of any individual asteroid. Eighty-five per cent of the asteroids have mean motions greater than twice and less than three times that of Jupiter; and the mean motions of none approximate closely either of these, the

two simplest ratios possible. The next simplest ratios lie beyond the limits of the zone; that is, there are no asteroids having mean motions nearly equal to or less than one and a half times that of Jupiter, and none nearly equal to or greater than four times that of Jupiter. The labor of determining the general perturbations and computing tables of an asteroid is as great as in the case of a major planet. It is no wonder, therefore, that tables have been prepared for scarce a dozen of these small bodies, and that these are already out of date.

Of well-known comets of short period, Encke's, which has the shortest period of any, possesses the greatest interest to the student of celestial motions, since it was from a discussion of the orbit of this comet that Encke detected evidence of the existence of a resisting medium which produces an acceleration in the comet's mean motion. This acceleration has been confirmed by the investigations of Von Asten and Backlund. The investigations of Oppolzer and Haerdtl indicate that there is an acceleration also in the mean motion of Winnecke's comet.

We have thus glanced briefly at the present condition of our knowledge of the motions of the principal bodies of the solar system. Only four cases have been found in which we cannot fully explain these motions, so far as known, by Newton's law of gravity. The unexplained discordances are the motion of the perihelion of Mercury, and the accelerations of the mean motions of the moon and the two periodic comets just named.

If we go beyond the solar system, we cannot tell whether Newton's law does or does not apply without modification to all parts of the universe. It is principally in the hope of answering this question that double-star observations are carried on; and, in the case of the many binary systems already detected, Newton's law is satisfied within the errors of observation. Nevertheless, this evidence is purely negative, and its value, it seems to me, not at all commensurate with the labor expended upon it, unless it be in the case of such objects as Sirius, whose observation may assist in the solution of the problem of irregular so-called proper motion. The angles subtended are in general so small that relatively large personal errors are unavoidable; so that, even though their motions be controlled by a law or laws of gravity widely different from that of Newton, it is not likely that such differences can be proved with any degree of certainty. It is rather to the study of the proper motions of the fixed stars and of the nebulae, and then only after a lapse of hundreds and perhaps thousands of years, that we must look for a solution of this question.

SOME PHASES IN THE PROGRESS OF CHEMISTRY.¹

SINCE the isolation of oxygen by Priestley, the search for new elements has been carried on vigorously, and the facilities for this pursuit have been much increased by the use of the delicate spectroscopic methods. The result has been to continually extend the list of bodies which are grouped under this head. The announcement of new discoveries during the last ten years has been especially large, over seventy bodies having been added to the list during this time. The largest number added by any observer has resulted from the joint labors of Krüss and Nilson on the absorption spectra of the rare earths, and reaches to over twenty. Should these discoveries be verified, the possible number of compounds which would result is something enormous, but, judging from experience, few are likely to survive a very searching inspection; yet one of them, 'germanium,' discovered by Winkler in 1886, has already been accepted as one of the missing elements in Mendelejeff's scheme, whose existence and properties he predicted.

Since the unit weight of hydrogen is taken as the standard for comparison, while the determination of the atomic weights of a large number of the elements has been made only through the intervention of oxygen, the ratio of the atomic weights of these two elements is the most important one to be determined, and many attempts have been made to solve this problem. The older experiments of Dumas and others were recently subjected to a careful scrutiny, and it was shown that they were not sufficiently exact. As

¹ Abstract of an address before the Section of Chemistry of the American Association for the Advancement of Science at Cleveland, O., Aug. 15-22, 1888, by C. E. Munroe, vice-president of the section.

the determination of atomic weights is of the greatest importance for the validity of the modern theories of chemistry, many experimenters of the greatest skill devoted themselves to researches on this subject, and, by means of improved methods, results of great accuracy were obtained. All these researches are of great intrinsic value and interest; but nevertheless they show, that even yet, with all the advantages of purity of material, perfection of apparatus, and precision of methods, united to great skill and extensive attainments on the part of the experimenter, the attractive hypothesis of Prout yet remains experimentally unproved. Many hold that the failure in the proof has been due to constant errors in the experimental processes; but Meyer and Seubert, from an elaborate discussion of the determinations of the atomic weight of silver and of those of the other more important elements calculated by its means, declare that they all contradict Prout's hypothesis in its characteristic original conception, and that it must therefore be looked upon as having been disproved by experiments.

Crookes suggests a hypothesis which may account for certain of the discrepancies in the atomic-weight determinations without resorting to the supposition of constant errors. He supposes that elements, instead of being composed of parts of matter which are identical throughout, are really composed of groups of particles which are only approximately alike, and whose weights only approximate to that average which we call the atomic weight. Hence it is possible that in different portions of such congeries different average values within small limits may obtain. Still it is remarkable that such close coincidences should result as have resulted from the observations made on material obtained from widely separated sources.

The determination of molecular weights is of nearly equal importance with that of the weights of the atoms. Thanks to Avogadro's law, we are able, when the substance can be obtained in the gaseous state, to determine its true molecular formula. When, however, the body cannot be completely volatilized unchanged, we have until recently been dependent upon isomorphism and the laws of molecular volumes and of specific heats, and upon analogical comparisons, to furnish us with estimates of the molecular weights. A new method of determining these weights was discovered by Raoult, who deduced a formula from the depression of the freezing-point of solutions. He showed, that by knowing the weights of the substance dissolved and of the solvent, and by knowing the depression of the freezing-point, the molecular weight may be calculated. He has examined a large number of substances whose molecular weights had previously been determined by their vapor density, and the results obtained illustrate in a remarkable manner accuracy and general application of this new method.

There has long existed a conviction in the minds of chemists that the molecular constitution of bodies in the solid state was much more complex than in the gaseous, owing to polymerization; and the opinion finds support in the diminishing density and increasing molecular simplicity of such bodies as acetic acid and sulphur when subjected to high temperatures. By analogy this aggregation of molecules should proceed as we pass from the gaseous through the liquid to the solid state. Is it not, then, singular that the molecular weights derived from Raoult's method for bodies in a state of solution should be identical, or approximately so, with those deduced from their densities in the state of a gas? This method fails to afford any indication whatever of this molecular complexity in solids and liquids. Must it not, then, be assumed that the solvent has effected the complete dissociation of the complex molecules present in it? If so, this probably extends to all cases of true solution without chemical action, if such there be; and this is assumed in this method, for, although the solvent used has been varied, it has given similar results.

Until recently we have known little precisely about the nature of solution. It has been held by some to differ essentially from chemical combination, but no satisfactory solution was offered until Mendelejeff made his important researches on this subject. He says, solutions may be regarded as strictly definite, atomic, chemical combinations at temperatures higher than their dissociation temperature. Definite chemical substances may be either formed or decomposed at temperatures which are higher than those at which dissociation commences. The same phenomenon occurs in solu-

tions: at ordinary temperatures they can be either formed or decomposed. In addition, the equilibrium between the quantity of the definite compound and of its products of dissociation is defined by the laws of chemical equilibrium, which laws require a relation between equal volumes and their dependence on the mass of the active component parts. Therefore, if the above hypothesis of solution be correct, comparisons must be made of equal volumes. The specific gravities are the weights of equal volumes; and, moreover, we must expect the specific gravities of solutions to depend on the extent to which the active substances are produced: therefore the expression for specific gravity, s , as a function of the percentage composition, p , must be a parabola of the second order, while between two definite compounds which exist in solutions we must expect that the differential co-efficient $\frac{ds}{dp}$ will be a rectilinear function of p .

This theory has been proved by experiment, and not a single case was found in which it did not hold good. Later on, Crompton and Mendelejeff extended this theory to the discussion of electric conductivity of aqueous solutions, and the results have been very encouraging, being entirely in favor of Mendelejeff's theory of solution. Thus it is shown that even this seemingly simple process is very complex, and it is in the study of such processes that probably the most important progress in the theory of chemical processes will be made. This study will lead us to a clearer understanding of the properties of matter.

The evidence supplied by the various branches of chemistry has forced the conviction in the minds of many of the ablest chemists, that all matter is one, and varies only as it is acted upon by force; while, on the other hand, the transformations of energy which are continually to be seen occurring in nature and in art, as continually prove the truth of that glorious conception, the doctrine of the conservation of energy, and equally force the conviction that all energy is one, and varies only in its manifestations.

The belief in the unity of matter is as old as philosophy, and, as has been said, this belief has in recent times been strengthened to conviction by the development of such facts as I have alluded to above; and this conviction has been supported by the more recently discovered evidence that the properties of the elements are functions of their atomic weights, and that the elements, when arranged according to their atomic weights, fall into natural and periodic groups; for it is a fundamental deduction from the law of periodicity, that the various elementary atoms must be aggregations or condensations of one and the same primordial substance. Strong as the conviction resting upon this evidence may be, there is yet lacking the crucial proof; for we have as yet failed to observe the passage of matter from the form of one elementary substance to that of another, or the resolution of any element into or its creation from primordial matter.

The case for the evolution of the elements from periodical matter has been very ably summed up by Crookes, while, in addition, he has brought forward experimental proof of the possible existence of bodies, which, though neither compounds nor mixtures, are not elements in the strictest sense of the word. These bodies, which he styles 'meta-elements,' consist of different groups, which shade off so imperceptibly the one into the other, that it is impossible to erect a definite boundary between any two adjacent bodies, and to say that the body on this side of the line is an element, while the one on the other side is non-elementary. Yet by means of fractionation these bodies may be separated one from the other, and then they exhibit slight spectral differences.

Finally Grünwald has announced that during a mathematical investigation of the changes which the properties, and especially the spectra, of two bodies undergo when they unite to form a new substance, he discovered a simple and important proposition of a future chemico-mathematical theory of perturbations; and by its means he has shown the compound nature of hydrogen and oxygen, and has demonstrated the dissociation of hydrogen in the sun. The method employed is a spectral one, and requires conditions which cannot be reproduced at the will of man; so that if it stands the tests of criticism, which is doubtful, it will not then enable us to witness the evolutionary process in actual operation.

Hence we find for the doctrine of evolution in the domain of chemistry, that the tests yield absolute results when applied to

compound matter; but the extension of the doctrine to the genesis of the elements is a pure speculation, and bids fair at present to be incapable of absolute proof.

ON THE INTERNATIONAL GEOLOGICAL CONGRESS, AND OUR PART IN IT AS AMERICAN GEOLOGISTS.¹

THIS association, at the meeting in Buffalo in 1876, appointed a committee to consider the propriety of holding an international congress of geologists at Paris during the international exhibition of 1878, for the settling of obscure points relating to geological classification and nomenclature.

Through the efforts and influence of this committee a congress was held in Paris in 1878, at which representatives from this country and from almost all the countries of Europe were present, and the business of the congress as indicated above was fairly begun. A second meeting was held at Bologna, Italy, in 1881; a third at Berlin in 1885, at which some progress was made; a fourth meeting is to be held in London in September of this year, and it is to be presumed that further progress will be made in the two important subjects before it, — classification and nomenclature.

But a meeting of the congress must be held in this country, and American geology must be fully represented, before any conclusions can be reached which will be accepted by the scientific world. At the meeting in London an effort will be made to have the next meeting, that of 1891, held in this country. There is good reason to ask that a meeting be held here before the discussions on the important topics under consideration are closed. We think our field of observation an important one, better than that of any of the countries of Europe, and perhaps better than all combined. This was the opinion of the older geologists; and such, too, is the opinion of many active geologists of the present day. Therefore we may look for the geological congress here three years from this time.

With this early notice of what is expected of us, it becomes us to make our preparations to show what we have done in geography and geology, and to enforce their claims to acceptance, as part of the material to be used in providing for uniform classification and names. As a profitable way of beginning our work, we inquire what are the points in each of these sciences which are settled, and what still remain to be worked out.

The foundation of all geological work is a good, reliable map of the country. Our country has greatly suffered from an inaccurate knowledge and description of our boundaries, in the north-east as well as in the south. Similar difficulties were encountered by the inaccuracy of surveys of State boundaries and land grants. It is true, these are not the points of interest in our association; but they furnish most potent reasons for making accurate maps, and they cause the supplies to be granted for making such maps. Good work in this line has been done by the Coast and Geodetic Survey and several other institutions, and its prosecution should be urged as rapidly as possible. But attention must be paid also to the topographic features, which are of equal value to the engineer, the farmer, the business-man, and the geologist.

The United States Geological Survey began systematic topographic work several years since, and it is now in progress in different sections of the United States. The maps are being engraved in the best manner, and issued as fast as they are completed.

We are far behind the countries of Europe in respect to maps of the whole country; but it is believed that our later maps will not suffer in comparison with the best of those of foreign lands, and, from some experience in directing such surveys, I feel warranted in saying that no public expense incurred in carrying on scientific explorations meets with such hearty recognition and approval as that for making and publishing such information in regard to the topographic features of the country in which we reside or travel. To us, however, geography is of most interest, because the forms and features of the earth's surface furnish a guide to direct us in our geological studies, and a means of recording their results with accuracy and clearness.

¹ Abstract of an address before the Section of Geology and Geography of the American Association for the Advancement of Science, at Cleveland, O., Aug. 15-22, 1888, by George H. Cook, vice-president of the section.

Geology, which treats of the structure of the whole earth, and which includes in its domain facts ascertained and principles deduced from all its parts, was first systematized from a very limited portion of the globe. It is not surprising that a system arranged consistently with the facts in a single country should not be comprehensive enough to meet the circumstances of all others. American geologists began by transferring the German, English, and French systems to this country. It took little time to find they did not fit the circumstances here; but, with that reverence for authority which is due from the younger to the older, we have been trying to make our geology conform to theirs. The effort is only partially successful, and we have to admit that something larger and more far-reaching must be devised before the science can be called a general one, applicable in all places.

It was probably some clear perception of this want in the science which led our fellow-members to move for an international congress of geologists, and now it is our part to see where the deficiency lies, and to do what we can to make preparations for supplying it.

The time is very short since geology was first studied in any systematic way in this country, and the advances have been rapid and large. From the time of Maclure's 'Observations on the Geology of the United States of America,' begun in 1809, and the establishment of Silliman's *American Journal of Science and Arts*, the growth of American geology has been rapid and plainly marked. The *American Journal* itself continues to be a repository of the advances of geological science. The Academy of Natural Sciences began the publication of geological papers the same year. During the ensuing twenty-three years, numerous surveys and reports were made, and the progress of geology was rapid.

On April 2, 1840, a meeting was held in Philadelphia, and the American Association of Geologists and Naturalists was organized. Of the eighteen present, thirteen or fourteen were geologists fresh from the field. The proceedings of the meeting, which was continued through a second and third day, are of interest to us as showing the problems which occupied them, something of the questions then settled, and of those on which they sought information and advice. Professor Hitchcock exhibited specimens of 'fossil footmarks so called,' and the association appointed a committee to visit the localities, and to report at the next meeting. The subject of diluvial action was discussed at this and the subsequent meetings.

Meetings were held by the association in the successive years, 1841 to 1847 inclusive, and it was then resolved into the American Association for the Advancement of Science, the first meeting of which was in 1848. The Section of Geography and Geology, now Section E of the American Association, is the representative of the society organized by American geologists to collate the individual work of each other, and to bring them into harmony of succession and name. It has already done much, and has reached the position from which it is prepared to do much more.

Many and perplexing questions have arisen in the progress of geology, some of which have taxed the powers of our ablest men. By continued efforts they are being solved. The Taconic question, the triassic formation of New Jersey, Pennsylvania, and the States farther south, the place of the American trias in the geological column, and other problems, received due attention at the meetings of the association. Some of these vexed questions were solved; in others considerable progress was made.

In the International Geological Congress the two topics for examination, and, if possible, for agreement, are the general system of nomenclature, and the colors to be used in making geological maps. It is, however, perfectly obvious, that, before agreeing on names to be used, the objects to be named must first be agreed upon; and it is evidently from the lack of completeness in the geological column in any single country where the geology has been well studied and described that the first difficulty arises. The order of succession of the rocks has been published, and names have been given to them; and, now that these have been in use, it is difficult to so change them as to make them a part of a scheme that shall be of universal application. It was this end which our association aimed at in their resolution passed in 1876; and, while progress has been made in the work at each meeting since held by the congress, it is